Does Compensation Disclosure Lead to Better Job Performance?

Evidence from the Mandatory CFO Pay Disclosure

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Abstract

The 2006 SEC rule, by changing the definition of Named Executive Officers, for the first time mandated the disclosure of CFO compensation. We exploit this setting and use a difference-in-differences research design to study the impact of CFO compensation disclosure on CFO job performance. We hypothesize that the disclosure of CFO compensation information, by facilitating shareholder monitoring of the board and motivating the board to improve CFO compensation contract design, leads to better CFO job performance in providing high-quality financial reports. Analyses support our prediction: The treatment firms, which, under the 2006 rule, start to disclose CFO compensation information, compared to the control firms, which were already disclosing CFO compensation prior to 2006, experience a significant improvement in financial report quality as exhibited in the reduced frequency of both accounting restatements and internal control weaknesses, as well as improved accrual quality. Further strengthening our conclusion, the improvement in CFO performance in financial reporting for the treatment firms is more pronounced for firms with younger CFOs, firms with CFOs subject to weaker internal monitoring, and firms facing higher litigation risk. We contribute to the disclosure literature by showing a causal impact of compensation disclosure on job performance. Our findings also have regulatory implications.

Keywords: SEC compensation disclosure mandate, accounting restatement, internal control quality, CFO performance.

JEL classification: M41.

1. Introduction

Compensation contract is a primary tool to incentivize managers to act in the shareholders' interest. Due to the dispersed ownership, a single shareholder may not be willing to incur the costs on its own to monitor and negotiate with managers as the benefits can be enjoyed by all shareholders. As a result, corporate boards are designated by the shareholders to design and enforce such contracts. However, even the board has agency conflict with shareholders because the board may be dominated or influenced by managers to design poorly incentivized contracts that enable managers to extract rent. These concerns motivate the Securities and Exchange Commission (SEC) to mandate the disclosure of management compensation as early as the Great Depression (Murphy, 2013) with the rationale that greater transparency and public oversight can mitigate the agency conflict between the board and shareholders. Despite the sound motivation, there lacks empirical evidence on whether compensation disclosure can improve compensation contracts for managers and in turn result in better job performance of managers.

This paper exploits the setting of the 2006 SEC rule, which for the first time mandated the disclosure of CFOs' (chief financial officers') compensation for all firms, and examines the causal impact of CFO compensation disclosure on CFO's job performance. Both before and since the 2006 rule, the SEC has been requiring firms to disclose compensation-related information for all Named Executive Officers (NEOs), but the 2006 rule changed the definition of NEOs from including the chief executive officer (CEO hereafter) and the four other most highly compensated executive officers to including the CEO, the CFO, and the three other most highly compensated executive officers. Thus, this change in the 2006 rule has a differential effect on two groups of firms: The firms that were already disclosing compensation-related information for CFOs before 2006 (control firms) continue to do so, and the firms that were not disclosing compensation-related

information for CFOs before 2006 (treatment firms) are now required to.[1] This setting allows a clear identification of the causal impact of the disclosure of CFO compensation information on the treatment firms, using the control firms as a benchmark.[2] By focusing on CFOs, some of whose compensation is for the first time being disclosed, we can address the causal question of whether compensation disclosure affects job performance.

Conceptually, the disclosure of compensation information can facilitate shareholder monitoring of the board in making compensation-related decisions and lead to a better alignment of interests between shareholders and the board and managers, thus reducing agency costs. Holmstrom (1979) shows theoretically that any information about the agent's actions can add value by making compensation contracts more efficient. In the setting of compensation disclosure, the information disclosed is about the board's actions in designing compensation schemes and incentivizing managers, and shareholders make use of such information in monitoring the board. Although there are no explicit contracts between shareholders and the board, the disclosure of compensation-related information may provide shareholders with information on the board's actions and aid shareholders in various activities that have corporate governance implications (e.g., class action lawsuits, shareholder proxy contests, say-on-pay votes, takeover bids, labor market decisions, or sale of shares).[3] That is, greater transparency of the board's action in designing CFO compensation contracts sheds light on the effectiveness of the board, which is otherwise not easily

[1] We are not aware of any firm that, prior to 2006, voluntarily disclosed CFO compensation information when the CFO was not among the four highest compensated executive officers.

[2] Note that although the 2006 SEC rule also requires more detailed disclosure of compensation-related information for all NEOs, such requirements affect both the treatment firms and the control firms to the same extent and thus do not affect our inference based on the comparison between the treatment firms and the control firms. In other words, the assumption required in our setting is that the change in financial reporting quality is the same for treatment and control groups due to the expanded disclosure requirement of the 2006 rule (see same point made by Armstrong and Kepler 2018), but not so for the treatment effect (i.e., the first-time disclosure of CFO compensation) under the 2006 SEC rule.

[3] See Armstrong et al. (2010) for a discussion of the role of informal contracts in mitigating agency conflicts between shareholders and the board.

observable by the shareholders (Gow et al., 2018). By increasing the reputation and litigation costs for the board, the enhanced transparency motivates the board to improve their effectiveness in CFO compensation design. Therefore, an application of Holmstrom's theory to the agency problem between shareholders and the board suggests that the disclosure of CFO compensation information facilitates shareholder monitoring of the board and reduces agency costs, ultimately leading to a better alignment of interests between shareholders and the board and managers and better CFO job performance.[4,5]

To measure CFO job performance, we focus on CFOs' performance in financial reporting, because financial reporting is CFOs' primary responsibility (e.g., Jiang et al., 2010; Kim et al., 2011). In addition, the SEC's decision to mandate compensation disclosure for CFOs is explicitly motivated by the CFO's role in financial disclosure: "We believe that compensation of the principal financial officer is important to shareholders because, along with the principal executive officer, the principal financial officer provides the certifications required with the company's periodic reports and has important responsibility for the fair presentation of the company's financial statements and other financial information."[6] Consistent with the SEC's argument, prior studies provide evidence that CFOs' compensation is associated with internal control quality (Wang, 2010; Hoitash et al., 2012) and financial reporting quality (Indjejikian and Matějka, 2009; Jiang et al., 2010).

[4] Murphy (2013) makes a similar prediction.

[5] For treatment firms, CFO's compensation contracts were not disclosed before the 2006 rule, which makes it difficult to directly examine the change in CFO compensation contracts around the compensation disclosure. Thus, we examine the effect of compensation disclosure on CFO job performance, an outcome of CFO compensation contracts. In Section 4.4, we provide evidence on improved CFO compensation contracts on a subsample of firms that explicitly discuss compensation contracts for CFOs.

[6] Source: page 117, the SEC Securities Act release No. 33-8732A. https://www.sec.gov/rules/final/2006/33-8732a.pdf

Using a sample of S&P 1500 firms from 2003-2012, we find that the treatment firms, compared to the control firms, exhibit a greater improvement in financial report quality, that is, a larger reduction in the frequency of accounting restatements and internal control weaknesses. The effects are both statistically significant and economically meaningful: After controlling for firm characteristics, the treatment firms' probability of accounting restatements (internal control weaknesses) decreased around the 2006 rule by 4.4% (1.9%) more than the control firms. The results hold when we use a short window of two years around the 2006 rule, an alternative sample period of 2003-2010 and when we exclude the financial crisis period (2007-2008).

Our identification strategy relies on the fact that the SEC rule prior to 2006 required the disclosure of CFO compensation if and only if the CFO was among the four most highly compensated non-CEO executive officers. A potential concern is that firm and CFO characteristics of the treatment firms are systematically different from those of the control firms, and such differences may explain the difference in the change in CFO performance around the 2006 rule for these two groups of firms. To address this concern, we conduct two further tests. First, we construct a propensity-score-matched (PSM hereafter) sample of control firms. Using the PSM sample (86 pairs of treatment and control firms), we continue to find that the treatment firms exhibit a more pronounced improvement in financial reporting quality around the implementation of the 2006 rule. Second, we identify an alternative group of control firms whose CFOs were the lowest paid among the top four non-CEO executives before the 2006 rule. Arguably, this group of CFOs is more similar to the CFOs in the treatment group in terms of their characteristics. Based on this alternative control group, we continue to find a greater improvement in financial reporting quality for the treatment group around the 2006 rule.

Our theoretical arguments suggest that compensation disclosure facilitates shareholder monitoring of the board's compensation practice, which encourages the board to improve its compensation design and provide more appropriate incentives for CFOs (incentive effect). To provide evidence on the said mechanism, based on the PSM sample, we manually collect the performance measures applicable to CFOs after the 2006 rule for both the treatment and control firms. Among these firms explicitly discussing performance measures applicable to CFOs, the number of performance measures applied to CFOs is higher for treatment firms than for control firms under the 2006 rule. To the extent that more performance measures indicate more efficient compensation contracts (Holmstrom, 1979), this descriptive summary suggests that after the first-time disclosure of CFO compensation for treatment firms, boards provide more efficient contracts for CFOs of treatment firms than of control firms under the 2006 rule, consistent with the mechanism we posit. In addition, we find that treatment firms are more likely to adopt clawback provisions in compensation contracts than the control firms after the 2006 rule. This evidence again is consistent with improved incentive and better compensation design for the treatment firms.

Other than the above mechanism, the disclosure of CFOs' compensation-related information can improve CFOs' performance through another mechanism: Compensation disclosure may expose firms with low-quality CFOs and force them to replace low-quality CFOs with high-quality candidates, which in turn leads to better performance (labor market effect). This is essentially a signaling effect, by disclosing compensation, lower-quality CFO who are paid less will be exposed to the market, and in anticipation of this adverse signaling, boards will replace these low-quality CFOs with high-quality CFOs to preempt criticism and scrutiny from the shareholders. Although both mechanisms are broadly consistent with compensation disclosure facilitating shareholder monitoring of the board and reducing agency costs, we are interested

primarily in the incentive effect. For this purpose, we identify treatment firms without CFO turnovers around the implementation of the 2006 rule by manually collecting the names of the CFOs who certify financial statements under Section 302 and 906 of Sarbanes-Oxley Act (SOX) in the two years before and two years after the 2006 rule. We find that the improvement in CFO performance in financial reporting still remains for treatment firms without CFO turnovers. Thus, our results of improved CFO performance is at least partly attributable to the incentive effect of CFO compensation disclosure.

Next, we predict that the impact of compensation-information disclosure on CFO performance varies with CFOs' implicit incentives, the internal monitoring of CFOs, and external litigation threats. First, younger CFOs have greater reputational concerns due to their longer horizons (e.g., Cheng, 2004). When faced with stronger shareholder monitoring under the 2006 rule, young CFOs are more likely to increase effort to improve performance in financial reporting. Second, CFOs subject to weaker internal monitoring from the audit committee before the 2006 rule are likely more sensitive to the increase in external shareholder monitoring under the 2006 rule and thus exhibit a larger improvement in job performance. Third, if a firm does not properly compensate and incentivize its CFO, shareholders may use the newly disclosed CFO compensation information required under the 2006 rule to aid in the litigation process, exposing the firm to greater litigation costs. Thus, firms facing higher ex-ante litigation risk before the rule are more likely to respond to the CFO compensation-information disclosure mandate with a better alignment between the CFO's compensation and his financial reporting duties, resulting in better performance. Results from cross-sectional analyses support these predictions: The improvement in CFOs' performance in financial reporting around the 2006 rule is more pronounced for firms

with younger CFOs, with CFOs subject to weaker pre-rule internal monitoring, and with higher pre-rule litigation risk.

Our study makes the following contributions. First, we add to the literature on the effect of disclosure. Although many studies show the effects of financial-information disclosure, evidence on the benefits of compensation-information disclosure is relatively scarce.[7] The relative scarcity of evidence on the impact of regulations on compensation-information disclosure is partly attributable to the difficulty in locating control groups that allow for a clear identification of the regulatory effects, which is a challenge that studies on regulatory effects commonly face (Leuz, 2007; Leuz and Wysocki, 2016). By exploiting the differential impact of the CFO compensation disclosure mandate on firms already disclosing CFO compensation and firms starting to disclose CFO compensation, we draw a causal inference of the disclosure of CFO compensation information on CFO job performance. Using the firms that were already required to disclose CFO compensation information prior to 2006 as a benchmark group, our DiD analyses show that firms that are required under the 2006 rule to start disclosing CFO compensation information exhibit an improvement in CFO performance in financial reporting relative to the control firms. Further strengthening our conclusion, cross-sectional analyses indicate that the improvement in CFOs' performance in financial reporting around the 2006 rule for the treatment firms varies with the characteristics of the firms and the CFOs in ways predicted by economic theories.

Our paper is related to Jin and Leslie (2003), who show that the 1998 Los Angeles County requirement that hygiene-quality grade cards be displayed in restaurant windows causes an improvement in restaurant hygiene quality. Whereas the disclosure requirement studied by Jin and

[7] Exceptions are Gipper (2017), Bloomfield (2018), Ferri et al. (2018), and Wang et al. (2018), all of which study the effects of the expanded compensation disclosure requirement under the 2006 SEC rule. Three of them examine the change in CEO compensation, whereas Ferri et al. (2018) examine the change in market reaction to earnings announcements (i.e., ERC).

Leslie (2003) is accompanied by an assessment of product quality (as in the hygiene grade cards issued by Department of Health Services), the disclosure requirement in our setting requires only the disclosure of compensation practice, without any associated quality assessment (i.e., what is good compensation practice vs. what is not).[8] Our findings suggest that disclosure of compensation information alone can facilitate shareholder monitoring and improve corporate performance in financial reporting, thus providing policy implications beyond Jin and Leslie.[9]

Our study is also related to Ferri et al. (2018), who hypothesize and find that the 2006 SEC compensation-disclosure rule reduces investors' uncertainty about managers' incentives and reporting objectives, as evidenced by an increase in the earnings response coefficient (ERC). By contrast, we extend the theory of Holmstrom (1979) and hypothesize that the disclosure of CFO compensation information facilitates shareholder monitoring on board's designing CFO compensation contracts and leads to better CFO job performance in providing high-quality financial reports. Although our empirical evidence is consistent with that of Ferri et al. in that rational investors will respond to higher reporting quality with a higher ERC, the two studies are conceptually different because investors' uncertainty about managers' reporting incentives (and their reaction to financial reports) can vary without any change in the underlying reporting quality. In fact, using discretionary accruals to measure reporting quality, Ferri et al. (Table 7) find no evidence of higher reporting quality following the 2006 rule. By contrast, we find robust evidence that the 2006 rule results in higher reporting quality, as reflected in less restatement, less internal control weakness, and lower discretionary accruals. The difference in findings between our study

[8] The SEC emphasized that the purpose of the 2006 rule is to make more compensation information available to shareholders, and that the SEC is not trying to set executive compensation or pass judgement on boards' compensation decisions (White, 2006).

[9] In a concurrent paper, Li and Xu (2016) show that the mandatory disclosure rule increases CFO pay level and turnover and conclude that the new rule results in greater agency costs for the firms affected. Our study complements Li and Xu (2016) by providing a more balanced view on the cost and benefit of this mandatory disclosure rule.

and Ferri et al. is potentially attributable to the different settings exploited by the two studies. Whereas both studies use a DiD research design to examine the effect of the 2006 rule, the identification strategies are different. Ferri et al.'s identification relies on different fiscal year-ends across firms, but our identification comes from the combined effect of (1) the 2006 rule mandating the disclosure of CFO compensation for all SEC registrants, and (2) a subset of these firms already disclosing CFO compensation information prior to 2006.[10] Our research design and findings are consistent with industry practitioners' view that the mandatory disclosure of CFO compensation is "a major benefit" of the 2006 rule (Harris, 2007). Ultimately, reporting quality is a different theoretical construct than investors' uncertainty about manager reporting incentives; thus, our study and Ferri et al. complement each other by documenting two distinct but non-mutually exclusive effects of the 2006 rule, one on corporate financial reporting behavior and the other on investors' perception of reporting objectives.

Second, our study adds to the burgeoning research on CFOs. Prior studies document a positive association between compensation incentive and CFO performance (Wang, 2010; Kim et al., 2011; Hoitash et al., 2012). Our study shows that the mere disclosure of CFO compensation information can have a significant impact on CFO's performance. Our evidence also suggests that the CFO plays a distinct and material role in determining financial reporting quality, thus adding to the literature on the relative importance of CEOs and CFOs in making financial reporting decisions (Jiang et al., 2010; Feng et al., 2011; Kim et al., 2011).

Last, our analyses indicate that the 2006 SEC rule, by requiring the disclosure of CFO compensation information, results in an improvement in CFO performance in financial reporting

[10] One feature of our treatment is that it represents a single homogenous treatment for the treatment group, whereas the treatment in Ferri et al. (2018) is better characterized as providing non-homogeneous treatments that depend on how particular firms endogenously choose to interpret and implement the rules (Armstrong and Kepler, 2018).

for firms that did not disclose CFO compensation information prior to 2006. This evidence has regulatory implications, especially given that the disclosure of compensation information has long been a favorite method used by the SEC and the Congress in response to public outcries about executive compensation in the U.S. (Murphy, 2013). In addition, the finding is informative to the SEC because the SEC explicitly states that CFOs' important role in financial disclosure motivates its decision to mandate CFO compensation disclosure.

The rest of the paper is organized as follows. Section 2 discusses the background information and develops the hypothesis. Section 3 describes the research design. Section 4 reports empirical results. Section 5 concludes.

2. Background and Hypothesis Development

The Securities and Exchange Commission's Securities Act release No. 33-8732A requires the expanded disclosure of information related to executive and director compensation starting from the end of 2006.[11] The SEC is "strongly committed to helping investors get the information they need (through required disclosure for public companies) about executive compensation so that shareholders and investors can judge that, however they choose and react however they like" (White, 2006).

The 2006 rules regarding compensation disclosure are different from the old rules in two aspects. First, more information (expanded) is required to be disclosed: the criteria used in reaching executive compensation decisions and the relation between the company's executive

[11] Compensation information is disclosed in annual proxy statements and annual reports on Form 10-K (for large public firms) or 10-KSB (for small businesses). Annual reports on Form 10-K or 10-KSB typically refer readers to the information in the annual proxy statement rather than presenting the information directly. Companies must comply with SEC Act release No. 33-8732A in Forms 10-K and 10-KSB for fiscal years ending on or after December 15, 2006 or in any proxy statements filed on or after December 15, 2006, that are required to include such information for fiscal years ending on or after December 15, 2006.

compensation practices and corporate performance. Second, the new rules change the definition of NEOs whose compensations are required to be disclosed from including CEO and the next four most highly compensated executive officers to including the CEO, CFO, and the next three most highly compensated executive officers.

The second change offers a nice setting for us to make a causal inference on how compensation disclosure affects job performance. Specifically, the firms that were already disclosing compensation-related information for CFOs before 2006 have to continue to do so, whereas the firms that were not disclosing compensation-related information for CFOs before 2006 are now required to. As such, we are able to identify a treatment group and a control group, and comparing the two can difference out any effect due to other confounding factors that affect both. Furthermore, by focusing on CFOs, we can tease out an important effect of the 2006 rule, because CFOs are of particular importance in financial reporting decisions (e.g., Jiang et al., 2010; Kim et al., 2011).

Extending agency theory (e.g., Holmstrom, 1979; Shavell, 1979), we contend that the disclosure of CFOs' compensation information facilitates shareholder monitoring of the board's actions in designing CFOs' compensation schemes, which in turn mitigates agency conflict between shareholders and the board, and ultimately results in better CFO incentives and performance. Holmstrom (1979) theory suggests that greater information that helps reveal managers' actions can improve the contract efficiency between shareholders and managers. In our setting of compensation disclosure under the 2006 rule, the information disclosed is about the board's actions in designing compensation schemes and incentivizing managers. The improved transparency of the board's actions in designing CFO compensation contracts sheds light on the effectiveness of the board, which is otherwise not easily observable by the shareholders (Gow et

al., 2018). Applying the logic in Holmstrom's theory, as long as the disclosure of compensation-related information provides shareholders with information on the board's actions, shareholders make use of such information in monitoring the board and conducting various activities that have corporate governance implications (e.g., class action lawsuits, shareholder proxy contests, say-on-pay proposals and votes, takeover bids, labor market decisions, or sale of shares). By increasing the reputation and litigation costs for the board, enhanced disclosure and shareholder monitoring could motivate the board to provide better compensation incentives for CFOs. Therefore, enhanced shareholder monitoring of the board's compensation practice is expected to encourage the board to improve its compensation design and provide more appropriate incentives for CFOs, resulting in better CFO performance.[12]

Based on the above arguments, we expect that the improvement in CFOs' job performance around the 2006 rule is greater for firms starting to disclose CFO compensation information (treatment group) compared to firms continuing to disclose CFO compensation information (control group). We thus hypothesize the following in the alternative form:

H1: Relative to the control group, firms in the treatment group exhibit a greater improvement in CFOs' job performance under the 2006 rule.

3. Sample and Research Design

We obtain the initial S&P 1500 firm list from ExecuComp and accounting information from Compustat, from CRSP the stock price and return data to compute variables based on stock returns,

[12] Gipper (2017) also proposes the improved transparency of pay details subject the board to greater shareholder monitoring, consistent with our arguments here. They study the effect of greater shareholder monitoring reflected in pay level for all NEOs, whereas we focus on better incentivized compensation contracts for CFOs (not necessarily higher pay) and better CFO performance. In an additional test, we control for the effect of pay increase on CFO performance.

from ISS the individual director data, and from Thomas Reuters the institutional holding data. Data on accounting restatements and internal control weaknesses are from Audit Analytics.

We first extract the list of S&P 1500 firms (past and present) for the year 2006 from Compustat and ExecuComp, which consists of 1,672 firms. For each firm, we search the 10-K filings and collect the names of the CFOs who certify the financial statements under Section 302 and 906 of SOX for the two most recent years ending before December 15, 2006 (the effective date of the 2006 rule). Having identified the certifying CFOs, we then look through the proxy statements (DEF14A forms) for the two consecutive years ending before December 15, 2006 to determine whether the CFOs' compensation was (not) disclosed in the pre-rule period. In this way, we do not need to rely on the job title in ExecuComp to determine the designated CFO of the firm and whether her/his pay was disclosed, which reduces coding errors.[13] Identifying CFOs manually also allows us to compile the list of firms with or without CFO turnover, as needed in later analysis. Among our initial sample of 1,672 firms, we exclude 247 firms that disclosed CFOs' compensation in one year but not in the other year before the 2006 rule, because we cannot consistently classify them as treatment or control firms. Of the remaining 1,425 firms, 146 (1,279) never (consistently) disclosed CFOs' compensation in the two consecutive years before the 2006 rule, and we thus classify them as treatment (control) firms. After excluding firms with missing financial data, our final sample to test H1 for the analysis of financial reporting and internal control quality includes 9,246 and 9,239 firm-year observations from 2003 to 2012, respectively, covering 1,183 unique firms, among which 109 (1,074) are treatment (control) firms.

3.1. Measures of CFOs' job performance

[13] ExecuComp provides compensation data, executive names, and their titles, as disclosed in proxy statements. In ExecuComp, CFOs' job titles are expressed by multiple terms, such as Chief Financial Officer, Chief Accounting Officers, and Vice President-Finance, which makes it difficult to determine who is the principal financial officer designated by the firm.

CFOs' primary role includes financial reporting and maintaining effective internal controls, and SOX Section 302 and 906 require CFOs to certify the financial statements and internal control. We thus follow prior literature and measure CFOs' performance by financial reporting and internal control quality (Bedard et al., 2014). Our first measure of CFOs' job performance is accounting restatement (excluding restatements that are due to a change in accounting standards or merger and acquisitions), which is an ex-post measure of poor financial reporting quality. Prior literature often uses accounting restatement to examine the association between financial reporting quality and managerial characteristics (e.g., Aier et al., 2005; Demerjian et al., 2013). We obtain non-reliance accounting restatements from Audit Analytics. *RESTAT* is an indicator variable equal to one if a firm's financial statements are subsequently restated, and zero otherwise.

Our second measure of CFOs' job performance is the effectiveness of internal control over financial reporting. Section 404 of SOX requires that firms attach an auditor-attested report that highlights material weaknesses in internal control to their 10-K filings. Auditing Standard No. 2 requires auditors to provide an opinion on a client's internal control over financial reporting. We obtain the Section 404 reports from the Audit Analytics and use two measures of internal control quality. Specifically, *ICW_D* is an indicator variable equal to one if a firm's auditor reports at least one material weaknesses in its internal control, and zero otherwise. *Ln*(*ICW_COUNT*) is log of one plus the count of material weaknesses in the auditors' internal control report required under Section 404.

3.2. Empirical models

Tests of accounting restatements

We augment the model in Cao et al. (2012) to test the effect of the 2006 rule on accounting restatement (H1):

$$RESTAT = \alpha_0 + \alpha_1 TREAT + \alpha_2 POST + \alpha_3 TREAT \times POST + \alpha_4 Ln(AT) + \alpha_5 LEV + \alpha_6 BUSSEG + \alpha_7 LOSS + \alpha_8 FOREIGN + \alpha_9 ROA + \alpha_{10} CURRENT + \alpha_{11} MERGER + \alpha_{12} FINANCING + \alpha_{13} RETVOL + \alpha_{14} BOARDSIZE + \alpha_{15} INDEPENDENT + \alpha_{16} INSIDER + Industry\ fixed\ effects + \varepsilon \quad (1)$$

where the dependent variable is an indicator variable *RESTAT*, an inverse measure of CFOs' job performance. *TREAT* equals one if a firm is a treatment firm, which did not disclose CFOs' compensation-related information in the proxy statements before the 2006 rule, and zero otherwise. *POST* equals one for firm-years with the fiscal yearends on or after December 15, 2006, and zero otherwise. Our variable of interest, *TREAT*×*POST*, captures the differential change in CFOs' job performance for treatment firms relative to control firms in the post-2006 period. H1 predicts the coefficient on *TREAT*×*POST* (α_3) to be negative, consistent with a greater improvement in CFOs' job performance for treatment firms than for control firms under the 2006 rule.

We control for various firm characteristics in prior studies (Cao et al., 2012; DeFond et al., 2016): firm size (*Ln(AT)*), leverage (*LEV*), operational complexity (*FOREIGN* and *BUSSEG*), and firm performance (*ROA* and *LOSS*). Dechow et al. (1996) find that firms with greater financing needs are more likely to conduct financial fraud, which results in the restatement of their financial statements. We thus control for merger and acquisitions (*MERGER*) and new financing (*FINANCING*). *MERGER* is an indicator variable equal to one if a firm is engaged in a merger or acquisition during the year, and zero otherwise. *FINANCING* is an indicator variable equal to one if the number of shares outstanding increases by at least 10 percent during the year, or if long-term debt increases by at least 20 percent during the year, and zero otherwise. Prior studies find that weak corporate governance negatively affects a firm's financial reporting quality, so we control for a number of corporate governance variables, including the proportion of independent directors

on board (*INDEPENDENT*), the number of directors on board (*BOARDSIZE*), and insider ownership (*INSIDER*). Finally, we include industry fixed effects in the model because the likelihood of accounting restatement may vary across industries. In all of our estimation analyses of CFOs' job performance, standard errors are clustered by firms.

Tests of internal control quality

We next estimate the following model in Ogneva et al. (2007) to test the effect of the 2006 rule on firms' internal control quality:

$$ICW = \alpha_0 + \alpha_1 TREAT + \alpha_2 POST + \alpha_3 TREAT \times POST + \alpha_4 BUSSEG + \alpha_5 FOREIGN + \alpha_6 MERGER + \alpha_7 RESTRUCTURE + \alpha_8 SALE_G + \alpha_9 INV + \alpha_{10} MKV + \alpha_{11} LOSS + \alpha_{12} ZSCORE + \alpha_{13} Ln(AGE) + Industry\ fixed\ effects + \varepsilon \quad (2)$$

where the dependent variable is alternately an indicator for internal control with material weaknesses (*ICW_D*) or the log of one plus the count of material weaknesses in a firm's internal control (*Ln(ICW_COUNT)*). When ICW is proxied by *ICW_D*, the logit model is used; when proxied by *Ln(ICW_COUNT)*, OLS estimation is used. A negative coefficient estimate on TREAT×POST is consistent with H1 that CFOs' job performance improves to a larger extent for treatment firms than for control firms.

Control variables are based on prior studies (e.g., Doyle et al., 2007; Bedard et al., 2014). Ge and McVay (2005) and Doyle et al. (2007) find that internal-control weakness is more likely for more complex firms. We, therefore, include *BUSSEG* and *FOREIGN* to control for a firm's operational complexity. *RESTRUCTURE* is an indicator variable equal to one if a firm has non-zero restructuring costs, and zero otherwise. We include *MERGER* and *RESTRUCTURE* because a firm's structural changes are related to its internal control quality (Doyle et al., 2007). Ogneva et al. (2007) document a negative association between a firm's accounting application

measurement risk and its internal control quality. We, therefore, include sales growth (*SALE_G*) and inventory level (*INV*) in the model. We include market value of equity (*MKV*), loss indicator (*LOSS*), and a firm's bankruptcy risk (*ZSCORE*) to control for a firm's size and financial health (Doyle et al., 2007). Finally, we control for a firm's age (*Ln(AGE)*), because Doyle et al. (2007) show that the likelihood of having material weaknesses is higher for younger firms.

4. Empirical Results

4.1. Univariate analyses

Table 1, Panel A presents the descriptive statistics for measures of CFOs' job performance and control variables. To mitigate the influence of extreme values, we winsorize all continuous variables at the 1st and 99th percentiles of their distribution in the sample. We find that 10.3 percent of the firm-years' financial statements are subsequently restated and 5.5 percent of the firm-years in our sample disclose at least one material weaknesses in internal control over financial reporting. For firms with material weaknesses in internal control, the number of weaknesses ranges from one to 11. Descriptive statistics of control variables are comparable to those reported in prior studies (e.g., Doyle et al., 2007; Cao et al., 2012).

Table 1, Panels B and C provide the correlation matrix for the samples of accounting restatement and internal control quality, respectively. We find a significant negative correlation between accounting restatement (*RESTAT*) and the post-rule indicator (*POST*). We also find a negative relation between *POST* with the two measures of internal control quality (*ICW_D* and *Ln(ICW_COUNT)*). These results suggest that firms in general exhibit better financial reporting quality in the post-2006 period, which makes it important to identify a control group to difference out the impact of contemporaneous events.

Table 1, Panel D reports the univariate test result of the difference in accounting restatements and internal control weaknesses in the pre- and post-2006 periods for treatment firms and control firms, respectively. Both groups exhibit a reduction in restatement probability following the 2006 rule. The reduction in the likelihood of accounting restatement is 0.050 (0.071-0.021) larger for treatment firms than for control firms, significant at the five percent level. For internal control quality, both treatment and control firms are less likely to disclose material weaknesses in internal control after the mandatory disclosure of CFO compensation information. The decreases in *ICW_D* and *Ln(ICW_COUNT)* for treatment firms are 0.065 and 0.096 greater than those for control firms, respectively, and are statistically significant. Overall, the univariate analysis provides evidence supporting H1 that treatment firms exhibit a greater improvement in CFOs' job performance than control firms under the 2006 rule.

4.2. Multivariate analyses

Table 2 presents the logit regression result of estimating Eq. (1). The coefficient estimate on the interaction *TREAT*×*POST* is -0.657, significant at the five percent level (z-statistics = -2.38), and the marginal effect on *RESTAT* is -0.044, which is slightly less than the economic magnitude obtained in the univariate analysis. The significantly negative coefficient on *TREAT*×*POST* supports H1 that the disclosure of CFO compensation information improves CFOs' job performance in financial reporting to a greater extent for treatment firms relative to control firms.[14] The coefficient on *POST*, which captures the time-series change in the likelihood of accounting restatement for control firms in the post-2006 period, is also significantly negative, consistent with the univariate results in Panel D of Table 1. The coefficient on *TREAT*, which captures the difference between treatment and control firms in the pre-2006 period, is significant, suggesting

[14] We obtain significant and negative coefficient on *TREAT*×*POST* when we estimate Eq. (1) using OLS regression. The same is true when we estimate Eq. (2) using OLS regression.

there is a pre-treatment difference between the treatment and control groups. However, this is not detrimental to our DiD design because DiD design allows a pre-treatment difference as long as it does not change without the treatment effect (parallel trend assumption). We also more formally test the parallel trend assumption employing a dynamic model in a later section.

Table 3 presents the regression results of estimating Eq. (2) using both measures of internal control quality. In Column (1), in which the dependent variable is an indicator for internal control weaknesses (*ICW_D*), the coefficient estimate on *TREAT*×*POST* is -1.127 (z-statistic = -3.02). The marginal effect on *ICW_D* is estimated to be -0.019, which is smaller than the economic magnitude shown in the univariate test. The significantly negative coefficient on *TREAT*×*POST* indicates that relative to control firms, treatment firms exhibit a greater improvement in internal control quality following CFO compensation disclosure in 2006, supporting H1. The coefficient on the main effect *POST* is also significantly negative, indicating that CFOs for control firms improve their performance in internal control quality under the 2006 rule, though the improvement is larger for treatment firms. Again, the existence of a difference between treatment and control firms before the 2006 rule, indicated by the significant coefficient on *TREAT*, does not violate the parallel trend assumption. Column (2) presents the regression results with the number of material weaknesses (*Ln(ICW_COUNT)*) as the dependent variable. The coefficient on *TREAT*×*POST* remains significantly negative, consistent with that reported in Column (1), suggesting that our results are robust to alternative measures of internal control quality.

Overall, the results in Table 2 and Table 3 provide evidence and support H1 that relative to control firms, treatment firms exhibit an improvement in CFOs' job performance after the disclosure of CFOs' compensation under the 2006 rule.

4.3. Alternative control firms

Propensity score matching

Because whether a firm's CFO was among the four highest-paid non-CEO executives whose compensation information was required to be disclosed before 2006 is not a random outcome, treatment firms and control firms could differ in terms of underlying characteristics. To address the endogeneity concern that certain firm characteristics related to CFOs' compensation disclosure may also affect firms' financial reporting quality, we adopt an alternative approach, namely, the propensity-score-matching (PSM) approach, to test H1. Specifically, we first estimate a logit model in Eq. (3) to determine the propensity score of being a treatment firm, using the latest firm characteristics prior to December 15, 2006. The firm characteristics are those that affect CFO compensation disclosure before 2006 as well as CFOs' job performance in the second stage (Shipman et al., 2017), including firm size (*Ln(SALE)*), market-to-book (*MTB*), leverage (*LEV*), firm performance (*ROA*), annual stock return and its volatility (*RETURN* and *RETVOL*), and institutional holdings (*INST*). We also control for a number of corporate governance variables, including CEO duality (*DUAL_D*), the percentage of independent directors on the board (*INDEPENDENT*), and the number of directors on the board (*BOARDSIZE*).

$$TREAT = \alpha_0 + \alpha_1 Ln(SALE) + \alpha_2 MTB + \alpha_3 LEV + \alpha_4 ROA + \alpha_5 RETVOL + \alpha_6 RETURN + \alpha_7 INST + \alpha_8 BOARDSIZE + \alpha_9 INDEPENDENT + \alpha_{10} DUAL\_D + Industry\ \ fixed\ \ effects + \varepsilon \quad (3)$$

Based on the propensity score obtained from the above model, we identify for each treatment firm in our sample a matched control firm with the closest propensity score. We then re-estimate the baseline models using the matched treatment and control firms.

The results are presented in Table 4. Panel A reports the logit regression results of Eq. (3). We find that larger firms (*Ln(SALE)*) and firms with fewer independent directors on board

(*INDEPENDENT*) are more likely to belong to the treatment group. Based on the fitted probability obtained from the logit model, we match each treatment firm to its closest control firm with a caliper of 0.1 and without replacement. This matching procedure yields 86 pairs of treatment and control firms. The univariate analysis (untabulated) shows that financial reporting quality is comparable for matched treatment and control firms in the pre-2006 period, but it is higher for treatment firms than for control firms in the post-2006 period. Panels B and C present the regression results of Eq. (1) and (2) for the PSM sample, respectively. Although the coefficient on *TREAT* turns to be statistically insignificant, we continue to find a significantly negative coefficient on *TREAT×POST* for both regressions, consistent with those reported in Tables 2 and 3 and supporting H1. The results suggest that the improvement in CFOs' job performance for treatment firms relative to control firms is not driven by the difference in firm characteristics between the two groups. The treatment and control groups no longer have a significant difference in CFOs' performance in the pre-2006 rule period, suggesting that the PSM procedure further eliminates pre-treatment differences. The lack of pre-treatment difference in the PSM sample compared to the larger, primary sample we employ also indicates that PSM procedure may sacrifice generalizability (Shipman et al. 2017).

An alternative group of control firms based on pre-rule CFO pay level

We also identify an alternative group of control firms based on the conjecture that the compensation level reflects CFO characteristics. Control firms' CFOs with compensation disclosed before the 2006 rule were among the four highest-paid non-CEO executives, whereas treatment firms' CFOs without compensation disclosed before the rule were not. One may argue these two groups of CFOs may have very different characteristics that likely drive the differential change in financial reporting quality between treatment and control firms.

To address these concerns, we use the compensation level (relative to the other three highest-paid executives) to proxy for the similarity in CFO characteristics. Specifically, we choose among the control firms a subgroup whose CFOs were the lowest paid among the top four non-CEO executives before 2006. Arguably, these CFOs are closer to treatment firms' CFOs who are not among the top four non-CEO executives. Based on this alternative group of control firms, as Table 5 shows, we continue to find a greater improvement in CFOs' performance, that is, a greater reduction in the likelihood of accounting restatements and having material weaknesses in internal control, for treatment firms than control firms after the 2006 rule.

4.4. Mechanisms causing CFO performance improvement

Further evidence on the incentive effect - better-incentivized compensation contracts for CFOs

As we argued earlier, one mechanism that CFO compensation disclosure improves CFO performance is by motivating the board to design better compensation contracts for CFOs. To provide evidence on the said mechanism, we conduct the following analyses. First, assuming more performance measures adopted for CFO compensation indicates greater contract efficiency (Holmstrom, 1979), we test whether treatment firms adopt more performance measures in CFOs' compensation contracts under the 2006 rule. Because the compensation contracts for CFOs of treatment firms were not disclosed before the 2006 rule, we focus on only CFOs' compensation contracts under the 2006 rule. We manually collect the performance measures applicable to CFOs from the proxy statements for two years after the 2006 rule. We start with the 86 pairs of treatment and control firms for our PSM sample. Table 6 shows a simple descriptive summary of the number of performance measures (including financial measures) applicable to CFOs after the 2006 rule for treatment and control firms separately. We find that the number of performance measures, as well as the number of financial measures, are greater for the treatment firms than for the control

firms under the 2006 rule, and such differences are statistically significant at 10% level.[15] Second, clawback provisions have been shown to improve financial reporting incentives and curb managers rent extraction (Chan et al. 2015), so clawback adoption can indicate boards doing a better job in designing the compensation contracts. Therefore, we further test whether clawback adoption is more prominent among the treatment firms than among the control firms. We rely on Corporate Library Database which provides clawback adoption information starting from 2007. Since we do not have the clawback adoption information prior to 2007, we only test whether more treatment firms adopt clawback provisions than control firms in the post-2006-rule period. We find that 47.99% of treatment firms adopt clawbacks in the period from 2007 to 2012, greater than that of the control firms (35.05%), and the difference is significant at 1% level. The result suggests that the boards adopt better compensation practice for the treatment firms than for the control firms.

CFO turnover – labor market effect vs. incentive effect

The results above provide evidence on the incentive effect, that is, after the mandatory disclosure of CFO compensation, the board may provide more appropriate incentives for CFOs, which results in improved CFO performance. Another possible channel through which CFO performance improves is that CFO compensation disclosure exposes poor-quality CFOs with lower pay in the labor market. Specifically, after the mandatory disclosure of CFO compensation, investors and boards learn about the pay level and performance measures in compensation contracts for CFOs of their firms as well as of peer firms; investors might use this information to infer about managers' ability and identify low-quality CFOs with lower pays. In anticipation of such adverse signaling from the compensation disclosure, boards are likely to replace poor-quality CFOs with more competent ones to preempt criticism and scrutiny from the shareholders, which

[15] The mean number of performance measures (financial measures) applicable to CFOs of control firms in the pre-2006 period is 3.00 (2.40), which is not statistically different from that for control firms in the post-2006 period.

in turn leads to better CFO performance (labor market effect). Consistent with the latter argument and the findings in Li and Xu (2016), we find that CFO turnover increases in the year immediately after the new compensation-disclosure rule, and more so for the treatment firms (19.86%) than for the control firms (14.70%). We calculate the ratio of CFOs' total compensation to the average of the other three non-CEO NEOs' for treatment firms with and without CFO turnovers in the pre- and post-2006 periods separately. Before the 2006 rule, CFOs' relative pay is comparable between the two groups of treatment firms, whereas under the 2006 rule, the relative pay of the incoming CFOs for treatment firms with CFO turnovers is higher than that for treatment firms without CFO turnovers. This result is consistent with the labor market effect that treatment firms likely replace poor-quality CFOs with more competent ones around the mandatory disclosure of CFO compensation.

Although both effects are broadly consistent with enhanced monitoring on the board by shareholders, we are primarily interested in the incentive effect. To assess whether the labor market effect drives our result, we re-run our main analyses by separating treatment firms with CFO turnovers from those without CFO turnovers. Specifically, using the hand-collected CFO names in the two years before and two years after the 2006 rule, we identify treatment firms with/without CFO turnovers within the four-year period.[16] We then re-estimate Eq. (1) and (2) for the subsample of treatment firms without CFO turnovers together with the control firms. Table 7 reports the regression results. In Panel A, for treatment firms without CFO turnover, the coefficient on *TREAT×POST* is significantly negative, indicating an improvement in CFO performance for treatment firms relative to control firms. Similarly, in Panel B, our result of the decreased likelihood of material weaknesses in internal control continues to hold for treatment firms without

[16] Results are similar if we identify treatment firms with and without CFO turnovers in the first year following the 2006 rule.

CFO turnovers. Thus, our results of improved CFO performance for treatment firms are not driven merely by the labor market effect, but at least partly attributable to the incentive effect.[17]

4.5. Cross-sectional analyses

CFOs' career and reputation concerns

The results above are consistent with our arguments that the disclosure of CFOs' compensation information for treatment firms, as required by the 2006 rule, facilitates external monitoring and contract efficiency in CFOs' compensation schemes, which provide incentives to CFOs to perform better. Prior literature suggests that executives in their early years of service have stronger incentives to build up their reputation for ability (Holmstrom, 1982; Cheng, 2004), and thus have greater incentives to improve performance when faced with enhanced external monitoring. We thus expect that the improvement in CFOs' performance caused by the 2006 rule is likely to be stronger for treatment firms with younger CFOs.[18] Because CFO turnover induced by the 2006 rule may affect firms' financial reporting and internal control, we consider only firms with the same CFO in the pre- and post-2006 periods (723 of 1,183 firms in our sample). We hand-collect CFOs' age from proxy statements and partition the 723 firms into two groups based on the age of CFOs. Columns (1) and (2) of Table 8 report the regression results for the subsamples, with the results for accounting restatement in Panel A and those for internal control quality in Panel B.[19] For both panels, the coefficient estimates on *TREAT*×*POST* are significantly negative only for firms with below-median-aged CFOs and insignificant for firms with above-median-aged CFOs. The finding that the improvement in CFOs' job performance for treatment firms around

[17] Untabulatd results show that for treatment firms with CFO turnovers, there is also an improvement in CFO performance, consistent with the labor market effect of replacing poor-quality with high-quality CFOs due to the adverse signaling effect arising from disclosure.

[18] Because the years of service as CFOs typically are not disclosed in their biographical information, we use CFOs' age as a proxy for the career and reputation concerns.

[19] For the purpose of brevity, for the analysis of internal control quality, we tabulate only the regression results based on *Ln(ICW_COUNT)*. Results for the other internal control quality measure (*ICW_D*) are qualitatively similar.

2006 rule is concentrated in the group of younger CFOs with higher reputation concerns is consistent with our argument that the disclosure of CFOs' compensation increases shareholder monitoring, which provides greater incentives for CFOs to improve performance.

Internal monitoring over CFO by audit committees

Audit committees play an important role in monitoring a firm's financial reporting quality as well as CFOs' performance (Klein, 2002; Farber, 2005; Badolato et al., 2014). When the internal monitoring from the audit committee was weak before the rule, the effect of external monitoring from shareholders will be more salient. If the CFO compensation disclosure under the 2006 rule enhances external shareholders' monitoring of the board in designing the compensation contract and thus improves CFO performance in financial reporting for the treatment group, we expect the improvement to be more pronounced for treatment firms with weak audit committees before the 2006 rule. We follow Kroos et al. (2018) and measure the power of the audit committee relative to that of the CFO to proxy for the audit-committee monitoring over the CFO (labeled as *POWER*). For each firm-year, we calculate it as the log of one plus the number of financial experts on the audit committee times the log of one plus the number of outside board seats of audit committee members deflated by the sum of one plus an indicator variable that equals one if the CFO is on the board, and zero otherwise. We then partition our sample into firms with weak (below-median) and strong (above-median) pre-2006 audit committee monitoring (*POWER*) and test the differential effect of CFO compensation disclosure on the two subsamples. Columns (3) and (4) of Table 8 report the regression results for the two subsamples. For both panels, the coefficients on *TREAT×POST* are significantly negative only for firms with weak audit committees and are statistically insignificant for firms with strong audit committees. The results provide evidence that in the post-2006 period, CFOs of treatment firms improve their performance relative to those of

control firms only when the audit committees are relatively weak and external shareholder monitoring plays a more important role. Because CFOs' impact on financial reporting is more salient for firms with weak audit committees (Beck and Mauldin, 2014), the cross-sectional analysis based on the monitoring of the audit committee also helps strengthen the argument that the improvement is the financial reporting is due to CFOs' better performance.

Ex-ante litigation risk

Prior literature finds that firms with accounting restatements are likely subject to litigation lawsuits, in which executives including CFOs are named as defendants (e.g., Palmrose and Scholz, 2004). In the post-2006 period, with more information on CFOs' compensation for treatment firms, investors can better monitor the boards' actions in designing CFO compensation schemes and incentivizing CFOs and take actions, including lodging litigation lawsuits against firms and CFOs. We thus expect that treatment firms with high litigation risk before the 2006 rule are more likely to better align CFOs' incentives with shareholders' under the 2006 rule, which may lead to greater improvement in financial reporting. We follow Kim and Skinner (2012) and calculate the ex-ante probability of litigation (*LITIGATION*) for each firm-year using the coefficient estimate from Table 7, column (3) in their paper. We then partition the firms on the basis of their average pre-rule *LITIGATION* into subsamples with high (above-median) and low (below-median) litigation risk and test the differential effect of CFO pay disclosure on the two subsamples. Columns (5) and (6) of Table 8 report the regression results. For the analyses of accounting restatement and internal control quality, the coefficients on *TREAT×POST* are significantly negative only for firms with high litigation risk and are insignificant for firms with low litigation risk. The results suggest that treatment firms exhibit an improvement in CFO performance under the 2006 rule only when they face high ex-ante litigation risk in the pre-rule period.

4.6. Robustness tests

Dynamic analysis

The validity of the DiD test relies on the parallel trend assumption: Absent the mandatory disclosure of CFO compensation under the 2006 rule, treatment firms would exhibit a time-series pattern in CFOs' job performance similar to that of the control firms. To examine the trend in CFOs' job performance between the two groups, we replace *POST* with five new indicators, $POST^{-1}$, $POST^{0}$, $POST^{1}$, $POST^{2}$, and $POST^{3+}$, and then interact them with *TREAT*. $POST^{-1}$ equals one for fiscal years that end between December 15, 2005, and December 15, 2006, and zero otherwise. $POST^{0}$ ($POST^{1}$, $POST^{2}$) equals one for fiscal years that end between December 15, 2006 (2007, 2008), and December 15, 2007 (2008, 2009), and zero otherwise. $POST^{3+}$ equals one for fiscal years that end after December 15, 2009, and zero otherwise. Table 9 reports the regression results for accounting restatements and internal control quality. In both regressions, the coefficient estimates on $TREAT \times POST^{-1}$ are statistically insignificant, suggesting the pre-treatment difference in CFOs' job performance between the treatment and control firms, captured by *TREAT*, remains constant prior to the mandatory disclosure of CFO compensation, consistent with the parallel trend assumption. More importantly, for the test of internal control quality (accounting restatement), the coefficients on the remaining interaction terms (except $TREAT \times POST^{1}$) are all significantly negative. The results suggest that the improvement in CFOs' job performance in treatment firms relative to control firms occurs only under the 2006 rule, providing evidence on a causal effect of CFO pay disclosure on CFOs' job performance. The above result also holds when we control for year fixed effects.

An alternative measure of CFOs' job performance

Jiang et al. (2010) find that CFOs' compensation is associated with accruals-based earnings management. We thus use accrual quality alternately to proxy for CFOs' job performance, and examine its relation to the disclosure of CFOs' compensation information for treatment firms. Specifically, we estimate the following accrual model developed by Dechow and Dichev (2002) and modified by McNichols (2002) and Francis et al. (2005) for each industry-year with at least 20 observations:

$$WC\_C_t = \alpha_0 + \alpha_1 CF_{t-1} + \alpha_2 CF_t + \alpha_3 CF_{t+1} + \alpha_4 SALE\_G_t + \alpha_5 PPE_t + \varepsilon_t \quad (4)$$

where WC_C is change in working capital accruals deflated by total assets. CF is cash flow from operations deflated by total assets. SALE_C and PPE are the change in sales and the current level of property, plant, and equipment, respectively. Absolute discretionary accruals (|*DA*|) is the absolute value of the estimated residual from Eq. (4).

Table 10 presents the regression results for the analyses of CFOs' job performance based on absolute discretionary accruals (|*DA*|). The coefficient estimate on the interaction *TREAT*×*POST* is -0.004 and is statistically significant with a t-statistic of -2.03, consistent with H1 that after the mandatory disclosure of CFO compensation, treatment firms exhibit an improvement in financial reporting relative to control firms. In addition, the coefficient estimate on the main effect, *POST*, is significantly negative, indicating that control groups exhibit less absolute discretionary accruals, that is, an improvement in financial reporting, in the post-2006 period.[20]

Change in CFOs' pay

[20] The reason we find a significant improvement in reporting quality whereas Ferri et al. (2018) do not is possibly because our identification of treatment firms is different from theirs (see our discussion in the introduction). Our results are not necessarily contrasting Ferri et al.'s, however, because Ferri et al. focus on the reduction in investors' uncertainty about managers' reporting incentive due to the 2006 rule, whereas we focus on the enhanced monitoring of the board effectiveness regarding CFO compensation design.

Li and Xu (2016) document a post-2006-rule increase in CFOs' total compensation. Gipper (2017) also documents an increase in all NEOs' compensation post the 2006 expanded disclosure requirement. To mitigate the concern that the increased pay itself without necessarily increasing the compensation contracting efficiency causes improved CFOs' job performance under the 2006 rule, we control for CFOs' pay level or change in CFO's pay around the 2006 rule in the main regression. For the treatment group prior to the 2006 rule, we use the lowest pay among the non-CEO executives as the pseudo pay of the CFO whose pay is not disclosed. The addition of this variable does not change our main inference. Therefore, the increase in CFO pay itself cannot explain our main results.

Other robustness tests

In the main analyses, we use a long window to increase the test power. However, one may be concerned that long window increases the chance of confounding factors at play. We, therefore, limit our sample to only one year before and one year after the 2006 rule. Our main results in Table 2 and 3 remain robust for the two-year sample. The main results also hold for a period-balanced sample from 2003 to 2010, which corresponds to four years before and four years after the 2006 rule. In another untabulated robustness test, we obtain results similar to those in Tables 2 and 3 when we remove the middle years (2007 and 2008, which correspond to the 2008 financial crisis) from our sample.

To further mitigate the concern that time-series change unrelated to the 2006 rule drives our results, we include year fixed effects and drop the stand-alone *POST* from the model. Results (untabulated) are qualitatively similar to those reported in the paper. The results are also robust to a model in which firm fixed effects replace industry fixed effects and the stand-alone *TREAT*, suggesting that our results are not driven by time-invariant, firm-specific omitted variables. The

firm-fixed-effect model supplements the PSM sample and further alleviates the concern that firm-specific characteristics, especially the difference between the treatment and control firms, explains our results. The baseline results also hold when we drop stand-alone *TREAT* and *POST*, and include both firm and year fixed effects.

5. Conclusion

In this study, we examine the effect of the CFO compensation disclosure mandate by the SEC in December 2006 on CFOs' job performance. Using hand-collected CFO data for S&P 1500 firms, we find that firms that did not disclose CFO compensation before the rule (treatment firms) experience a larger decrease in the likelihood of accounting restatement and internal control weaknesses under the 2006 rule relative to the firms that already disclosed CFO compensation before the rule (control firms), indicating that CFO compensation disclosure improves CFOs' job performance. The results continue to hold for the subsample of treatment firms without CFO turnovers, indicating that improved CFO performance is not driven by firms replacing low-quality CFOs with more competent ones around the pay disclosure. These findings support the notion that the additional information required by the SEC 2006 rule, that is, CFOs' pay level and its determinants, facilitates shareholder monitoring of the board in providing better incentives to CFOs, resulting in better CFO performance. Cross-sectionally, we find that the improvement in CFOs' performance is more salient for firms with younger CFOs, with CFOs subject to weaker internal monitoring by audit committees before the 2006 rule, and firms with higher ex-ante pre-rule litigation risk.

Our study substantiates the intended benefits of compensation disclosure regarding CFOs by the SEC. It also contributes to the disclosure literature, specifically the benefits of mandating the disclosure of compensation information.

Appendix Variable Definitions

Variable	Definition
Main variables of interest	
RESTAT	An indicator variable equal to one if a firm's financial statements are restated, and zero otherwise.
ICW_D	An indicator variable equal to one if a firm discloses at least one material weakness in its internal control reported under SOX Section 404, and zero otherwise.
Ln(ICW_COUNT)	Natural log of one plus the count of material weaknesses in a firm's internal control reported under SOX Section 404.
TREAT	An indicator variable equal to one if a firm did not disclose its CFO's compensation in the proxy statements for the latest two years before the 2006 rule (treatment firm), and zero otherwise
POST	An indicator variable equal to one for firm-years with fiscal year ending on or after December 15, 2006, and zero otherwise.
Other variables	
BOARDSIZE	The number of directors on board.
BUSSEG	Natural log of the number of a firm's business segments.
CF	Operating cash flow deflated by total assets.
CFVOL	Standard deviation of CF in the past five years.
CURRENT	The ratio of current assets to total assets.
DA	Signed discretionary accruals, which is the residual from the following equation developed by Dechow and Dichev (2002) and modified by McNichols (2002): $WC\_C = \alpha + \alpha_1 CF_{t-1} + + \alpha_2 CF_t + \alpha_3 CF_{t+1} + \alpha_4 SALE\_G + \alpha 5\ PPE + \varepsilon$, where *WC_C* is change in working capital accruals deflated by total assets, *CF* is cash flow from operations deflated by total assets, *SALE_G* is the changes in sales, and *PPE* is the current level of property, plant, and equipment divided by total assets.
|DA|	Absolute value of discretionary accruals (*DA*).
DUAL_D	An indicator variable equal to one if the CEO is also the chairman of the board, and zero otherwise.
FINANCING	An indicator variable equal to one if the number of shares outstanding increases by at least 10 percent during the year, or if long-term debt increases by at least 20 percent during the year, and zero otherwise.
FOREIGN	The sum of the sales from foreign segments deflated by the sum of sales of all segments.
INDEPENDENT	The number of independent directors on board divided by the number of directors on board.
INSIDER	The number of shares owned by individuals holding 5% or more divided by number of shares outstanding.
INST	Number of shares owned by total institutional investors divided by number of shares outstanding.
INV	Inventory divided by total assets.
LEV	Long-term debt plus short-term debt divided by total assets.
LITIGATION	The probability of being sued in year y+1, estimated based on the model in Table 7, Column 3 of Kim and Skinner (2012). More specifically, SUEProb / (1-SUEProb) = exp(-7.883 + 0.566*FPS + 0.518*ta + 0.982*salegrw + 0.379* return - 0.108*skewness + 25.635*stdev + 0.00007*turnover), where FPS is an indicator variable for industries that belong to biotech (SIC codes 2833-2836 and 8731-8734), computers (3570-3577 and 7370-7374), electronics (3600-3674), or retail (5200-5961). ta is total assets of year y; salegrw is the sales change from year y-1 to year y scaled by total assets of year y-1; return is the 12-month market-adjusted return in year y; and skewness and stdev are the stock-return skewness and standard deviation in year y. Turnover is trading volume in year y scaled by the beginning-of-year y shares outstanding (divided by 1,000).

Appendix (Cont'd)

Variable	Definition
Other variables (Cont'd)	
Ln(AT)	Natural log of total assets.
Ln(AGE)	Natural log of the number of months since the firm exists in CRSP.
LOSS	An indicator variable equal to one if a firm reports a net loss in the current year, and zero otherwise.
MERGER	An indicator variable equal to one if a firm is engaged in a merger or acquisition during the year, and zero otherwise.
MKV	Natural log of market value of equity.
MTB	The ratio of market value of equity to book value of equity.
PPE	Net value of property, plant, and equipment divided by total assets.
POWER	Natural log of one plus the number of financial experts on the audit committee times natural log of one plus the number of outside board seats of audit committee members deflated by the sum of one plus an indicator variable equal to one if the CFO is on board, and zero otherwise.
RESTRUCTURE	An indicator variable equal to one if a firm has non-zero restructuring costs, and zero otherwise.
RETURN	Stock return in the past 12 months.
RETVOL	Standard deviation of monthly stock return in the past 12 months.
ROA	The ratio of earnings before extraordinary items to total assets.
SALE_G	Change in sales divided by sales in the previous year.
ZSCORE	Decile rank of Altman's (1968) Z-score to be scaled between zero and one, where Z-score is calculated by 0.717× working capital / total assets + 0.847 × retained earnings / total assets + 3.107 × earnings before interest and taxes / total assets + 0.42 × book value of equity / total liabilities + 0.998 × sales / total assets. The higher the value, the greater the bankruptcy risk.

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Table 1 Sample Description and Univariate Analyses

Panel A: Descriptive statistics

Variable	Mean	Median	Std	Q1	Q3
Sample for accounting restatement (N=9,246)					
RESTAT	0.103	0.000	0.304	0.000	0.000
TREAT	0.092	0.000	0.290	0.000	0.000
POST	0.614	1.000	0.487	0.000	1.000
Ln(AT)	7.560	7.449	1.577	6.458	8.578
MERGER	0.173	0.000	0.379	0.000	0.000
FINANCING	0.193	0.000	0.395	0.000	0.000
BUSSEG	1.151	1.386	0.645	0.693	1.609
FOREIGN	0.311	0.212	0.332	0.000	0.539
MTB	2.886	2.192	2.380	1.479	3.395
LEV	0.201	0.192	0.162	0.041	0.315
ROA	0.041	0.051	0.097	0.020	0.088
CURRENT	0.251	0.232	0.160	0.123	0.341
LOSS	0.166	0.000	0.372	0.000	0.000
RETVOL	0.026	0.023	0.012	0.017	0.032
BOARDSIZE	9.140	9.000	1.878	8.000	10.000
INDEP	0.766	0.778	0.111	0.714	0.846
INSIDER	0.014	0.003	0.038	0.001	0.009
Sample for internal control weaknesses (N = 9,239)					
ICW_D	0.055	0.000	0.228	0.000	0.000
Ln(ICW_COUNT)	0.053	0.000	0.244	0.000	0.000
TREAT	0.089	0.000	0.284	0.000	0.000
POST	0.763	1.000	0.425	1.000	1.000
BUSSEG	1.148	1.386	0.647	0.693	1.609
FOREIGN	0.365	0.000	0.482	0.000	1.000
MERGER	0.362	0.000	0.481	0.000	1.000
RESTURCTURE	0.583	1.000	0.493	0.000	1.000
SALE_G	0.083	0.069	0.196	-0.011	0.155
INV	0.110	0.082	0.112	0.018	0.163
MKV	7.687	7.614	1.587	6.584	8.719
LOSS	0.163	0.000	0.370	0.000	0.000
ZSCORE	0.507	0.556	0.319	0.222	0.778
Ln(AGE)	5.590	5.568	0.737	5.106	6.148

Table 1 (Cont'd)

Panel B: Pairwise correlation among variables used in regression of accounting restatement

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
RESTAT	(1)																
POST	(2)	**-0.05**															
TREAT	(3)	0.01	0.00														
Ln(AT)	(4)	**-0.04**	**0.12**	**0.13**													
MERGER	(5)	-0.02	-0.01	**-0.03**	0.01												
FINANCING	(6)	**0.02**	**-0.04**	-0.02	**0.04**	**-0.22**											
BUSSEG	(7)	0.00	-0.01	**-0.04**	**0.14**	**0.03**	**-0.02**										
FOREIGN	(8)	-0.01	**0.06**	0.00	**0.08**	**0.04**	**-0.04**	**0.02**									
MTB	(9)	**-0.05**	**-0.09**	**0.03**	-0.01	-0.01	**0.07**	**-0.10**	**0.03**								
LEV	(10)	**0.02**	**0.03**	0.01	**0.35**	0.01	**0.10**	**0.05**	**-0.14**	**0.03**							
ROA	(11)	**-0.07**	**-0.03**	**0.04**	**0.16**	0.02	0.01	-0.01	0.02	**0.25**	**-0.18**						
CURRENT	(12)	0.01	**-0.02**	0.00	**-0.20**	0.00	**0.02**	-0.01	**0.08**	**-0.05**	**-0.16**	**0.02**					
LOSS	(13)	**0.07**	**0.04**	**-0.03**	**-0.21**	**-0.04**	-0.01	**-0.04**	**0.04**	**-0.11**	**0.08**	**-0.71**	0.01				
RETVOL	(14)	**0.05**	**0.21**	**-0.04**	**-0.37**	**-0.04**	0.01	**-0.09**	**0.05**	**-0.14**	**-0.04**	**-0.42**	**0.11**	**0.45**			
BOARDSIZE	(15)	**-0.04**	**0.06**	**0.07**	**0.51**	**-0.03**	0.02	**0.08**	0.01	0.01	**0.23**	**0.03**	**-0.05**	**-0.08**	**-0.21**		
INDEPENDNET	(16)	**-0.03**	**0.25**	**-0.03**	**0.18**	0.00	-0.02	**0.07**	**0.09**	0.00	**0.11**	**-0.03**	-0.04	0.00	-0.01	0.17	
INSIDER	(17)	**0.02**	**-0.04**	0.02	**-0.15**	0.02	0.00	**-0.02**	**-0.11**	0.00	**-0.07**	**0.04**	**0.05**	**-0.02**	**0.04**	**-0.15**	**-0.19**

Table 1 (Cont'd)

Panel C: Pairwise correlation among variables used in regressions of internal control quality

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
ICW_D	(1)													
Ln(ICW_COUNT)	(2)	**0.91**												
POST	(3)	**-0.16**	**-0.16**											
TREAT	(4)	0.01	0.02	0.00										
BUSSEG	(5)	0.01	0.01	**-0.02**	**-0.05**									
FOREIGN	(6)	0.04	**0.04**	**0.06**	0.01	**-0.05**								
MERGER	(7)	-0.01	-0.01	0.00	**-0.04**	**0.04**	**0.07**							
RESTRUCTURE	(8)	**0.02**	**0.02**	**0.05**	-0.01	**0.05**	**0.21**	**0.08**						
SALE_G	(9)	0.01	0.00	**-0.19**	-0.02	**-0.02**	-0.01	**0.16**	**-0.14**					
INV	(10)	0.01	0.01	0.01	0.01	**-0.05**	-0.01	**-0.06**	**-0.02**	**-0.03**				
MKV	(11)	**-0.14**	**-0.13**	**0.04**	**0.10**	**0.06**	**0.02**	0.02	0.01	**0.10**	**-0.17**			
LOSS	(12)	**0.12**	**0.12**	**0.05**	**-0.04**	**-0.03**	**0.06**	-0.01	**0.16**	**-0.22**	**0.02**	**-0.36**		
ZSCORE	(13)	**-0.07**	**-0.08**	**-0.08**	0.02	**-0.09**	**0.07**	**0.04**	**-0.13**	**0.18**	**0.10**	**0.13**	**-0.34**	
Ln(AGE)	(14)	**-0.07**	**-0.06**	**0.16**	0.02	**0.16**	-0.01	**-0.08**	**0.05**	**-0.14**	0.01	**0.26**	**-0.09**	**-0.12**

Panel D: Univariate analysis of the effect of CFO pay disclosure on accounting restatement and internal control quality

	Variable	Pre-2006 -rule (*POST*=0)	Post-2006-rule (*POST*=1)	Difference (Difference-in-differences)
Treatment firms	*RESTAT*	0.153	0.083	0.071 ***
	ICW_D	0.169	0.026	0.144 ***
	Ln(ICW_COUNT)	0.200	0.028	0.173 ***
Control firms	*RESTAT*	0.115	0.094	0.021 ***
	ICW_D	0.115	0.036	0.079 ***
	Ln(ICW_COUNT)	0.111	0.034	0.077 ***
Difference (Difference-in-differences)	*RESTAT*	0.038 **	-0.012	0.050 **
	ICW_D	0.054 *	-0.010	0.065 **
	Ln(ICW_COUNT)	0.089 **	-0.006	0.096 **

This table reports the descriptive statistics for measures of accounting restatement, internal control quality, and control variables. Panel A presents the descriptive statistics of variables used in regressions; Panels B and C present the pairwise correlations among variables in the regression of accounting restatement and internal control quality, respectively; Panel D compares the mean accounting restatements and internal control quality in the pre- and post-2006-rule periods for treatment and control firms, respectively. A correlation coefficient in boldface indicates a significance level of 0.05 or lower using two-tailed tests. Two-sample t-tests are used to test the differences in means. *** indicates a significance level at 0.01. All variables are defined in Appendix.

Table 2 Effect of CFO Pay Disclosure on CFOs' Performance in Accounting Restatement

	Dependent var. = *RESTAT*
TREAT	0.387*
	(1.96)
POST	-0.246***
	(-2.79)
TREAT×POST	-0.657**
	(-2.38)
Ln(AT)	-0.066*
	(-1.67)
MERGER	-0.079
	(-0.77)
FINANCING	0.158*
	(1.71)
BUSSEG	0.183**
	(2.40)
FOREIGN	0.122
	(0.77)
MTB	-0.082***
	(-3.68)
LEV	0.907***
	(2.95)
ROA	0.014
	(0.03)
CURRENT	0.215
	(0.56)
LOSS	0.320**
	(2.33)
RETVOL	1.571
	(0.42)
BOARDSIZE	-0.046
	(-1.54)
INDEPENDENT	-0.242
	(-0.61)
INSIDER	0.562
	(0.56)
Constant	-2.165***
	(-2.93)
Observations	9,246
Industry FE	Yes
Pseudo R^2	0.038

This table presents the logit regression result of the effects of CFO pay disclosure on CFOs' performance measured by accounting restatement. The z-statistics are reported in parentheses. Standard errors are clustered by firms. *, **, *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.

Table 3 Effect of CFO Pay Disclosure on CFOs' Performance in Internal Control Quality

	(1)	(2)
Dependent var. =	*ICW_D*	*Ln(ICW_COUNT)*
TREAT	0.738***	0.185***
	(2.89)	(3.01)
POST	-1.505***	-0.084***
	(-12.53)	(-8.79)
TREAT×POST	-1.127***	-0.178***
	(-3.02)	(-2.96)
BUSSEG	0.378***	0.018***
	(3.11)	(3.10)
FOREIGN	0.432***	0.025***
	(3.10)	(2.87)
MERGER	-0.084	-0.013**
	(-0.67)	(-2.03)
RESTRUCTURE	0.034	-0.007
	(0.25)	(-1.08)
SALE_G	0.471*	0.008
	(1.79)	(0.51)
INV	1.184	0.074
	(1.39)	(1.30)
MKV	-0.421***	-0.017***
	(-8.19)	(-6.07)
LOSS	0.528***	0.050***
	(3.53)	(3.96)
ZSCORE	-0.722***	-0.055***
	(-3.23)	(-3.92)
Ln(AGE)	-0.077	-0.002
	(-0.85)	(-0.41)
Constant	-11.076***	0.298***
	(-13.44)	(4.22)
Observations	9,239	9,239
Industry FE	Yes	Yes
Pseudo/Adj. R^2	0.172	0.075

This table presents the regression results of the effects of CFO pay disclosure on CFOs' performance in internal control quality. Logit regression is used when the dependent variable is *ICW_D*; OLS estimation is used when the dependent variable is *Ln(ICW_COUNT)*. The t-statistics (z-statistics) are reported in parentheses for OLS (logit) regressions. Standard errors are clustered by firms. *, **, *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.

Table 4 Effect of CFO Pay Disclosure on CFOs' Performance for the Propensity-Score-Matched Sample

Panel A: Estimation of the probability of being a treatment firm

	Dependent var. = *TREAT*
Ln(SALE)	0.420***
	(3.43)
MTB	0.019
	(0.33)
LEV	-0.696
	(-0.69)
ROA	2.576
	(1.07)
RETVOL	-18.278
	(-0.66)
RETURN	0.096
	(0.18)
INST	-0.379
	(-0.89)
BOARDSIZE	0.051
	(0.69)
INDEPENDENT	-2.184**
	(-2.31)
DUAL_D	-0.391
	(-1.44)
Constant	-15.851
	(-0.02)
Observations	1,183
Industry FE	Yes
Pseudo R^2	0.128

Table 4 (Cont'd)

Panel B: Effect of CFO pay disclosure on accounting restatement for the PSM sample

	Dependent var. = *RESTAT*
TREAT	0.365
	(1.12)
POST	0.084
	(0.28)
TREAT×POST	-0.792**
	(-1.97)
Ln(AT)	-0.114
	(-1.22)
MERGER	0.167
	(0.58)
FINANCING	0.456
	(1.63)
BUSSEG	0.126
	(0.68)
FOREIGN	0.137
	(0.31)
MTB	-0.154**
	(-2.22)
LEV	0.542
	(0.51)
ROA	1.348
	(0.71)
CURRENT	0.582
	(0.57)
LOSS	0.315
	(0.84)
RETVOL	2.614
	(0.25)
BOARDSIZE	0.116
	(1.52)
INDEPENDENT	-2.314**
	(-2.45)
INSIDER	-0.945
	(-0.51)
Constant	-1.570
	(-1.11)
Observations	1,415
Industry FE	Yes
Pseudo R^2	0.086

Table 4 (Cont'd)

Panel C: Effect of CFO pay disclosure on internal control quality for the PSM sample

	(1)	(2)
Dependent var. =	*ICW_D*	*Ln(ICW_COUNT)*
TREAT	0.632	0.036
	(1.42)	(1.56)
POST	-1.443***	-0.017
	(-3.80)	(-1.17)
TREAT×POST	-1.191*	-0.040*
	(-1.85)	(-1.69)
BUSSEG	0.210	-0.010*
	(0.74)	(-1.86)
FOREIGN	0.167	-0.003
	(0.38)	(-0.28)
MERGER	0.005	0.004
	(0.02)	(0.49)
RESTRUCTURE	0.260	0.002
	(0.58)	(0.29)
SALE_G	2.641**	0.022
	(1.98)	(1.01)
INV	-3.691	-0.105*
	(-1.21)	(-1.96)
MKV	-0.568***	-0.007**
	(-4.68)	(-2.47)
LOSS	0.917**	0.030*
	(2.04)	(1.68)
ZSCORE	-1.304**	-0.010
	(-2.14)	(-0.77)
Ln(AGE)	0.175	-0.003
	(0.61)	(-0.71)
Constant	1.042	0.132***
	(0.54)	(2.80)
Observations	1,353	1,353
Industry FE	Yes	Yes
Pseudo/Adj. R^2	0.293	0.075

This table presents the regression results of the effects of CFO pay disclosure on CFOs' performance for the propensity-score-matched sample. Panel A presents the regression result of a logit model for the probability of being a treatment firm, used to obtain the propensity scores; Panel B presents the regression result of the effects of CFO pay disclosure on accounting restatement; Panel C presents the regression results of the effects of CFO pay disclosure on internal control quality. Logit regression is used when the dependent variable is *RESTAT* or *ICW_D*; OLS estimation is used when the dependent variable is *Ln(ICW_COUNT)*. The t-statistics (z-statistics) are reported in parentheses for OLS (logit) regressions. Standard errors are clustered by firms. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.

Table 5 Effect of CFO Pay Disclosure on CFOs' Performance based on Alternative Control Firms

Panel A: Effect of CFO pay disclosure on accounting restatement

	Dependent var. = *RESTAT*
	Subsample of treatment firms and alternative control firms
TREAT	0.480**
	(2.01)
POST	-0.127
	(-0.71)
TREAT×POST	-0.800**
	(-2.51)
Constant	-14.135***
	(-11.29)
Observations	2,337
Control var.	Yes
Industry FE	Yes
Pseudo R^2	0.071

Panel B: Effect of CFO pay disclosure on internal control quality

	(1)	(2)
Dependent var. =	*ICW_D*	*Ln(ICW_COUNT)*
	Subsample of treatment firms and alternative control firms	
TREAT	0.186***	0.790***
	(2.94)	(2.65)
POST	-0.082***	-1.359***
	(-4.00)	(-6.33)
TREAT×POST	-0.188***	-1.261***
	(-2.98)	(-3.02)
Constant	0.100	-11.351***
	(1.53)	(-7.22)
Observations	2,316	2,316
Control var.	Yes	Yes
Industry FE	Yes	Yes
Pseudo/Adj. R^2	0.083	0.171

This table presents the regression results of the effects of CFO pay disclosure on CFOs' performance for a subsample of treatment firms and control firms whose CFOs are the lowest paid among the four non-CEO executives before 2006. Panel A presents the regression result of the effects of CFO pay disclosure on accounting restatement; Panel B presents the regression results of the effects of CFO pay disclosure on internal control quality. Logit regression is used when the dependent variable is *RESTAT* or *ICW_D*; OLS estimation is used when the dependent variable is *Ln(ICW_COUNT)*. The t-statistics (z-statistics) are reported in parentheses for OLS (logit) regressions. Standard errors are clustered by firms. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix

Table 6 Descriptive on the Number of Performance Measures in CFO Compensation Contracts

	Post-2006-rule period	Mean number of performance measures	Mean number of financial measures
Treatment firms	N=51	3.216	2.373
Control firms	N=42	2.548	2.143
Difference		0.668*	0.230*

This table presents the descriptive summary on the number of performance measures adopted in CFOs' compensation contracts for treatment and control firms separately in the post-2006 period. We focus on firms that explicitly discuss performance measures applicable to CFOs in their proxy statements. Two-sample t-tests are used to test the differences in means. * indicates a significance level at 0.10.

Table 7 Effect of CFO Pay Disclosure on CFOs' Performance for Treatment Firms without CFO Turnovers

Panel A: Effect of CFO pay disclosure on accounting restatement

	Dependent var. = *RESTAT*
	treatment firms without CFO turnovers
TREAT	0.198
	(0.81)
POST	-0.246***
	(-2.77)
TREAT×POST	-0.625*
	(-1.74)
Constant	-2.215***
	(-3.00)
Observations	8,900
Control var.	Yes
Industry FE	Yes
Pseudo R^2	0.039

Panel B: Effect of CFO pay disclosure on internal control quality

	(1)	(2)
Dependent var. =	*ICW_D*	*Ln(ICW_COUNT)*
	treatment firms without CFO turnovers	
TREAT	0.541	0.120
	(1.60)	(1.54)
POST	-1.504***	-0.119***
	(-12.50)	(-10.33)
TREAT×POST	-1.857***	-0.127*
	(-2.70)	(-1.60)
Constant	-11.589***	0.432***
	(-13.52)	(5.26)
Observations	8,907	8,907
Control var.	Yes	Yes
Industry FE	Yes	Yes
Pseudo/Adj. R^2	0.171	0.105

This table presents the regression results of the effects of CFO pay disclosure on CFOs' performance for treatment firms without CFO turnovers. Panel A presents the regression results of the effects of CFO pay disclosure on accounting restatement; Panel B presents the regression results of the effects of CFO pay disclosure on internal control quality. Logit regression is used when the dependent variable is *RESTAT* or *ICW_D*; OLS estimation is used when the dependent variable is *Ln(ICW_COUNT)*. The t-statistics (z-statistics) are reported in parentheses for OLS (logit) regressions. Standard errors are clustered by firms. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.

Table 8 Cross-Sectional Analyses

Panel A: Cross-sectional analyses of accounting restatement

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var. = *RESTAT*					
	Young CFO	Old CFO	Weak audit committee	Strong audit committee	High litigation risk	Low litigation risk
TREAT	0.375	0.332	0.356	0.427	0.451*	0.430
	(1.14)	(0.60)	(1.52)	(1.14)	(1.76)	(1.36)
POST	-0.312*	-0.101	-0.328***	-0.203	-0.388***	-0.175
	(-1.66)	(-0.66)	(-2.69)	(-1.48)	(-2.89)	(-1.40)
TREAT×POST	-1.507***	0.085	-0.731**	-0.558	-0.741**	-0.439
	(-2.85)	(0.17)	(-2.06)	(-1.18)	(-2.06)	(-1.01)
Constant	-1.398*	-13.437***	-2.481***	-13.739***	-12.759***	-2.856***
	(-1.84)	(-11.22)	(-3.15)	(-11.76)	(-10.96)	(-3.30)
Observations	2,769	2,952	4,562	4,684	4,607	4,639
Control var.	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.095	0.062	0.043	0.055	0.063	0.045

Panel B: Cross-sectional analyses of internal control quality

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent var. = *Ln(ICW_COUNT)*					
	Young CFO	Old CFO	Weak audit committee	Strong audit committee	High litigation risk	Low litigation risk
TREAT	0.167**	0.058	0.163**	0.004	0.123*	0.044
	(2.22)	(1.39)	(2.10)	(0.20)	(1.86)	(1.15)
POST	-0.051***	-0.033***	-0.102***	-0.036***	-0.079***	-0.060***
	(-3.79)	(-4.11)	(-6.78)	(-5.61)	(-6.09)	(-6.07)
TREAT×POST	-0.162**	-0.065	-0.173**	-0.008	-0.113*	-0.047
	(-2.18)	(-1.47)	(-2.19)	(-0.37)	(-1.74)	(-1.19)
Constant	0.220***	0.256***	0.231***	0.231***	0.384***	0.153***
	(4.91)	(3.95)	(4.35)	(7.62)	(6.89)	(4.02)
Observations	2,769	2,953	4,640	4,599	4,676	4,563
Control var.	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.059	0.054	0.076	0.035	0.063	0.051

This table presents the cross-sectional variation in the effects of CFO pay disclosure on CFOs' performance. Panel A presents the cross-sectional variation in the effects of CFO pay disclosure on accounting restatement; Panel B presents the cross-sectional variation in the effects of CFO pay disclosure on internal control quality. In Columns (1) and (2), the sample is partitioned at the median value of firms' mean CFO age; in Columns (3) and (4), the sample is partitioned at the median value of firms' mean power of the audit committee relative to that of CFO (*POWER*); in Columns (5) and (6), the sample is partitioned at the median value of firms' average litigation risk (*LITIGATION*). Logit regression is used when the dependent variable is *RESTAT*; OLS estimation is used when the dependent variable is

Ln(ICW_COUNT). The t-statistics (z-statistics) are reported in parentheses for OLS (logit) regressions. Standard errors are clustered by firms. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.

Table 9 Dynamic Analysis

	(1)	(2)
Dependent var. =	*RESTAT*	*Ln(ICW_COUNT)*
TREAT	0.458**	0.189***
	(2.07)	(2.65)
$POST^{-1}$	0.056	-0.046***
	(0.48)	(-3.93)
$POST^{0}$	-0.200	-0.078***
	(-1.55)	(-5.92)
$POST^{1}$	-0.473***	-0.097***
	(-3.25)	(-7.34)
$POST^{2}$	-0.477***	-0.132***
	(-2.91)	(-9.13)
$POST^{3+}$	-0.158	-0.115***
	(-1.35)	(-8.77)
$TREAT \times POST^{-1}$	-0.136	-0.007
	(-0.39)	(-0.12)
$TREAT \times POST^{0}$	-0.757*	-0.141*
	(-1.84)	(-1.93)
$TREAT \times POST^{1}$	-0.359	-0.178**
	(-0.80)	(-2.46)
$TREAT \times POST^{2}$	-1.819**	-0.205***
	(-2.44)	(-2.81)
$TREAT \times POST^{3+}$	-0.620*	-0.187**
	(-1.78)	(-2.54)
Constant	-2.065***	0.317***
	(-2.77)	(4.42)
Observations	9,246	9,239
Control var.	Yes	Yes
Industry FE	Yes	Yes
Pseudo/Adj. R^2	0.049	0.081

This table presents the regression results of the dynamic analysis of CFO pay disclosure on CFO performance. $POST^{-1}$ equals one for fiscal years that end between Dec. 15, 2005, and Dec. 15, 2006, and zero otherwise; $POST^{0}$ ($POST^{1}$, $POST^{2}$) equals one for fiscal years that end between Dec. 15, 2006 (2007, 2008), and Dec. 15, 2007 (2008, 2009), and zero otherwise; $POST^{3+}$ equals one for fiscal years that end after Dec. 15, 2009, and zero otherwise. Logit regression is used when the dependent variable is *RESTAT*; OLS estimation is used when the dependent variable is

Ln(ICW_COUNT). The t-statistics (z-statistics) are reported in parentheses for OLS (logit) regressions. Standard errors are clustered by firms. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.

Table 10 Effect of CFO Pay Disclosure on CFOs' Performance in Accruals Quality

Dependent var. =	|*DA*|
TREAT	0.004*
	(1.77)
POST	-0.003***
	(-3.66)
TREAT×POST	-0.004**
	(-2.03)
Ln(AT)	-0.001**
	(-1.97)
CFVOL	0.200***
	(14.43)
SALEVOL	0.045***
	(11.07)
CYCLE	-0.005***
	(-3.05)
LOSS	0.015***
	(8.78)
SALE_G	0.006***
	(4.61)
RETURN	0.002**
	(2.29)
Constant	0.017***
	(6.35)
Observations	8,218
Industry FE	Yes
Adj. R^2	0.281

This table presents the OLS regression results of the effects of CFO pay disclosure on CFO performance measured by absolute discretionary accruals. The t-statistics are reported in parentheses for OLS regressions. Standard errors are clustered by firms. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively. Variables are defined in Appendix.